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XIV.—*The Inconsistencies of Utilitarianism as the Exclusive Theory of Organic Evolution.* By Rev. JOHN T. GULICK\*.

*Natural Selection an Exclusive Theory with some Biologists.*

IN a previous article entitled "Divergent Evolution and the Darwinian Theory" † I dwelt chiefly on the need of a bionomic theory that should explain polytypic as well as monotypic evolution. One of the chief deficiencies in Darwin's discussion of the 'Origin of Species' is that he does not distinguish with sufficient clearness the conditions that are necessary for the transformation of an original species into a new species, when the former disappears in the process, leaving the latter to occupy its place, and the conditions that are necessary for the production of two or more species from one original species. In this paper it may be instructive to examine a vigorous attempt that has been made so to expound the theory of natural selection (which Darwin considered as inadequate to cover all the forms of monotypic evolution), that it shall serve as the full explanation of both monotypic and polytypic evolution in all organisms lower than man. By

\* From the 'American Journal of Science,' July 1890, pp. 1-14.

† Amer. Journ. Sci. vol. xxxix. pp. 21-30; Ann. & Mag. Nat. Hist. ser. 6, vol. v. p. 156.

confining our attention to Mr. Wallace's very interesting and suggestive volume on 'Darwinism' we shall be better able to judge of the possibility of producing a self-consistent theory on this basis; but we should bear in mind that the same view is maintained by many naturalists, and that parallel statements abound in their writings. Mr. Wallace's volume not only embodies the mature reflections of one of the joint authors of the theory of natural selection, but it fairly represents that phase of biological theory which considers diversity of natural selection through exposure to different environments the only cause of divergence. The following passage will show the exclusive nature of his theory:—"A great body of facts on the one hand and some weighty arguments on the other alike prove that specific characters have been and could only have been developed and fixed by natural selection because of their utility. We may admit that among the great number of variations and sports which continually arise many are altogether useless without being hurtful; but no cause or influence has been adduced adequate to render such characters fixed and constant throughout the vast number of individuals which constitute any of the more dominant species" ('Darwinism,' p. 142). This is in strong contrast with the following passage from the close of the Introduction of the sixth edition of the 'Origin of Species,' which is the last one that received the revision of the author:—"I am fully convinced that species are not immutable, but those belonging to what are called the same genera are lineal descendants of some other and generally extinct species, in the same manner as the acknowledged varieties of any one species are the descendants of that species. Furthermore I am convinced that Natural Selection has been the most important, but not the exclusive, means of modification." On page 421 of the same edition Darwin calls attention to the fact that this passage has "been placed in a most conspicuous position" in the different editions of his work, and complains of the writers who misrepresent his conclusions on this point.

*Facts that are neglected or denied.*

Though Darwin maintains that besides the inherited effects of use and disuse and the direct action of the external conditions there are other forms of variation leading to permanent modifications of structure independently of natural selection ('Origin of Species,' 6th London ed. p. 421), he does not attempt to explain how these divergences arise. Neither Darwin nor Wallace appears to have observed that, as in domestication, the isolated breeding of other than average

forms, in whatever way it is secured, is the one necessary and always effective cause of divergence, so, in nature, wherever there arises the isolated breeding of other than average forms, there divergence will be produced; or that, as exposure to different environments is only one of the causes that lead isolated bands of men to desire and select different types of variation in the same species of animal, so exposure of wild species to different environments is only one of several classes of causes that may subject isolated portions of one of these species to different forms of selection, producing divergence; or, again, that as differences in the uses to which men put an animal are not necessarily useful differences, so the differences in the uses which isolated portions of a species make of the environment, though they produce diversity of natural selection, leading to permanent divergence, are not necessarily useful differences. These, with other allied doctrines, which were presented in my paper on "Divergent Evolution through Cumulative Segregation," have received adverse criticism from Mr. Wallace in the work mentioned above. He says:—"In Mr. Gulick's last paper (Journ. of Linn. Soc., Zoology, vol. xx. pp. 189-274) he discusses the various forms of isolation above referred to under no less than thirty-eight different divisions, with an elaborate terminology, and he argues that these will frequently bring about divergent evolution without any change in the environment or any action of natural selection. The discussion of the problem here given will, I believe, sufficiently expose the fallacy of his contention; but his illustrations of the varied and often recondite modes by which practical isolation may be brought about may help to remove one of the popular difficulties in the way of the action of natural selection in the origination of species" (note on p. 150).

In this passage Mr. Wallace seems to take issue with each and all of my propositions; but after a careful study of his whole discussion one cannot but be in doubt whether he fully dissents from any of them. This uncertainty arises either from his failing to recognize distinctions which I have made, or from ambiguities and inconsistencies in his own statements.

*Extending the meaning of Natural Selection does  
not save the Theory.*

He represents me as contending that divergent groups are frequently found in which the action of natural selection is wanting. He here fails to distinguish between the absence of diversity in the action of natural selection and the absence of

any action of the same principle. I have never maintained that any species can long escape the action of natural selection; but I have that natural selection cannot produce transformation of a race unless it secures the propagation of other than average forms of that race; that it cannot be a cause of divergence unless to this condition is added the independent generation (*i. e.* isolation) of groups that are subjected to some diversity in its action; and that, in isolated groups, some of the divergent characters may be due to other causes of transformation. In the passage I have quoted from p. 142 he expresses great confidence in the proof that all specific characters are developed and fixed by natural selection; but in the discussion that follows concerning the influence of natural selection he claims as belonging to this principle sets of influences which are usually included under sexual selection and which he cannot regard as due to the reactions between the species and its environment (see 'Darwinism,' pp. 282-285), and even then it is found too narrow to cover all the facts of specific divergence; for when he comes to consider the origin and development of accessory plumes he has to abandon the theory to which he has clung through the greater part of the book. Speaking of the enormously lengthened plumes of the "bird of paradise and of the peacock," he says, on page 293, "The fact that they have been developed to so great an extent in a few species is an indication of such perfect adaptation to the conditions of existence, such complete success in the battle of life, that there is, in the adult male at all events, a *surplus of strength, vitality, and growth-power, which is able to expand itself in this way without injury.* That such is the case is shown by the great abundance of most of the species which possess *these wonderful superfluities* of plumage. . . . *Why, in allied species, the development of accessory plumes has taken different forms, we are unable to say, except that it may be due to that individual variability* which has served as the starting-point for so much of what seems to us to be strange in form or fantastic in colour, both in the animal and vegetable world." (The italics are mine.) According to the theory he has elsewhere maintained, *these superfluities* of form and colour which are not controlled by natural selection should present "a series of inconstant varieties mingled together, not a distinct segregation of forms" (p. 148); but in this passage he teaches that they have assumed different forms in allied species. On p. 141 he maintains that characters which are neither beneficial nor injurious are from their very nature unstable and cannot become specific, while here he offers a suggestion as to how they have become specific. There is, then, a problem

that presses for solution, namely the explanation of permanent divergence in characters that are useless without being hurtful (p. 142), unless he considers his suggestion "that it may be due to individual variability" an adequate explanation; and I presume he does not. On page 142 he says of characters that are "useless without being hurtful." "No cause or influence has been adduced adequate to render such characters fixed and constant;" but in speaking of "the delicate tints of spring foliage and the intense hues of autumn" he says, "As colours they are unadaptive and appear to have no more relation to the well-being of the plants themselves than do the colours of gems and minerals. We may also include in the same category those algæ and fungi which have bright colours—the red snow of the Arctic regions, the red, green, or purple seaweeds, the brilliant scarlet, yellow, white, or black Agarics, and other fungi. All these colours are probably the direct results of chemical composition or molecular structure, and being thus normal products of the vegetable organism need no special explanation from our present point of view; and the same remark will apply to the varied tints of the bark of trunks, branches, and twigs, which are often of various shades of brown and green, or even vivid reds or yellows" (p. 302). He here seems to admit that instead of useless specific characters being unknown they are so common and so easily explained by "the chemical constitution of the organism" that they claim no special attention.

*Inconsistency in extending the meaning of Environment.*

If Mr. Wallace accepts the definition of natural selection which makes it the survival of those members of a species which are best fitted to its environment (and this is the scope he seems to assign to it in the earlier half of Chapter V., where the matter is under special discussion), then he ought to admit that changes in a species produced by the action of the members of the species on each other although they are adaptive are not due to natural selection. If, on the other hand, natural selection is made to include the actions and reactions of the species on itself (and this he does on pages 282–285), then certainly he ought to admit that there may be changes in the action of natural selection without any change in the relations of the species to the environment. One way to escape this dilemma is to extend the definition of the environment, so as to include every influence that affects the species, whether it is within the species or external to it; but this reduces his

doctrine that without change in the environment there is no change in the organism to the fruitless truism that without some cause there is no change in the organism. An example of Mr. Wallace's extending the meaning of the environment so as to include the action of the members of a species on each other is found on page 149. After mentioning several arguments intended to show the impossibility that isolated portions of a species should diverge while exposed to the same environment, he remarks, "It is impossible that the environment of the isolated portion can be exactly like that of the bulk of the species. It cannot be so physically, since no two separated areas can be exactly alike in climate and soil; and, even if they are the same, the geographical features, size, contour, and relation to winds, seas, and rivers would certainly differ. Biologically the differences are sure to be considerable. The isolated portion of a species will almost always be in a much smaller area than that occupied by the species as a whole, hence it is *at once in a different position as regards its own kind.*" He then enumerates several differences in the biological environment that are liable to occur; but the point I wish now to note is that he mentions as one of the differences in the environment the "*different position as regards its own kind.*" This is exactly the difference which, in so far as it is the prevention of intercrossing and the consequent unification of endowments and habits, constitutes isolation; and unless he is able to show that this difference is incapable of producing any divergence, his contention is unsustainable. But he here yields the point at issue by mentioning this amongst the effective differences. The only way to escape the force of his concession is to claim, as he virtually does here, that isolation, being the separation of the isolated fragment from the influence of the original stock, is in itself a difference in the environment. By taking this position, however, he involves himself in another contradiction, for, if isolation is a difference in the environment, why does he deny that it has a direct influence in producing change in the organism?

*Diversity of Natural Selection during exposure to the  
same Environment.*

Another discrepancy in Mr. Wallace's theory is that, while he rightly assigns great importance to diversity of natural selection arising from divergent habits in appropriating the resources of the same environment, exhibited by different sections of the same species occupying the same area, he nevertheless insists that the representatives of a species, iso-

lated in different areas of the same environment, will be necessarily subjected to the same influences from natural selection, and will inevitably maintain the same characters and, of course, the same habits. That he believes divergent habits may arise, when the divergent groups are occupying the *same area*, and are prevented from crossing simply by the divergence of habits, will be seen by the case of the varieties of wolves mentioned on p. 105 and by some of the cases mentioned on pp. 108 and 117; also by the statement, on p. 119, that "When one portion of a terrestrial species takes to a more arboreal or a more aquatic mode of life the change of habits itself leads to the isolation of each portion," and by a similar statement at the bottom of p. 145. That he believes there can be no change either of habits or structure when portions of the same species are isolated in *different areas* under the same environment appears from the statement on p. 149 that "If the average characters of the species are the expression of its exact adaptation to its whole environment, then, given a precisely similar environment, and the isolated portion will inevitably be brought back to the same average of characters." And this he maintains will be the case even "if we admit that, when one portion of a species is separated from the rest, there will necessarily be a slight difference in the average character of the two portions."

*Does the difference in the Environment increase  
with each successive Mile?*

If the divergences presented by the Sandwich-Island land-molluscs are wholly due to exposure to different environments, as Mr. Wallace argues on pages 147-150, then there must be completely occult influences in the environment that vary progressively with each successive mile. This is so violent an assumption that it throws doubt on any theory that requires such support. Of all the suggestions made by Mr. Wallace concerning possible and inevitable differences in the environments presented in the successive valleys, it seems to me not one meets the requirements of the case or throws any light on the subject. The one suggestion which is quite applicable as an explanation is the one already quoted, that "the isolated portion is at once in a different position as regards its own kind." This is, I believe, a most potent difference, which (as Mr. Wallace's language seems to indicate) is directly introduced by isolation, and (adhering to the meaning usually given to environment) is not at all due to difference in the environments presented in the different areas.

*Unstable Adjustments disturbed by Isolation.*

There is a sentence in another chapter of Mr. Wallace's book which attributes to isolation (though without recognizing the important results that must follow) just that kind of influence in introducing a certain class of physiological divergences, which I claim for it in introducing not only physiological, but also psychological and morphological divergences. I claim that there is in many species more or less variation with unstable adjustment in the habits which determine what forms of food it shall appropriate, and that, when a few individuals of such a species (the offspring perhaps of a single female) are isolated, this adjustment is often so disturbed by the failure of the few individuals to completely represent the average character of the species and by their being freed from competition and wide interbreeding with those of their own kind that divergent habits of feeding are formed. I further claim that for the production of this result it is not at all necessary that the environments presented in the isolated districts should differ in any respect. Indeed, if all but one pair of a variable species should be destroyed, the descendants of that pair, remaining in the same area and under the same environment, would probably differ more or less from the original stock. Those that breed together must have habits that enable them to do so; and the offspring of those that interbreed widely will for the most part inherit the powers and habits that enabled their ancestors to interbreed widely; but if the offspring of a single family are carried to an isolated area presenting the same environment, there will be nothing to ensure the perpetuation of exactly the original powers and habits, unless the power of heredity is such that each pair is sure to transmit the complete average character of the whole species; and this is not the condition of all species that pair, if of any. Within the limits of each freely interbreeding portion of a species a mutual harmony and adjustment of habits is preserved, because it is the condition of propagation within those limits; but between portions that are prevented from interbreeding there is nothing but heredity to prevent divergence in the kinds of adjustment; and in variable species the probability is that divergence will in time show itself more or less distinctly. Though Mr. Wallace considers this reasoning fallacious when applied to divergence in habits, he uses an exactly parallel reasoning in the portion of the following passage which I designate by italics:—*It appears as if fertility depended on such a delicate adjustment of the male and female elements to each other that, unless constantly kept up by*



*the preservation of the most fertile individuals, sterility is always liable to arise. . . . So long as a species remains undivided and in occupation of a continuous area its fertility is kept up by natural selection; but the moment it becomes separated, either by geographical or selective isolation, or by diversity of station or of habits, while each portion must be kept fertile inter se, there is nothing to prevent infertility arising between the two separated portions.* As the two portions will necessarily exist under somewhat different conditions of life, and will usually have acquired some diversity of form and colour—both which circumstances we know to be either the cause of infertility or to be correlated with it—the fact of some degree of infertility usually appearing between closely allied but locally or physiologically segregated species is exactly what we should expect” (pp. 184–185). Notwithstanding this statement he does not seem to have grasped the idea that in the geographically isolated portions as well as in the others the “different conditions of life” of which he speaks may be the different relations to the environment into which the separated portions are brought by their divergent habits, without any reference to inevitable differences in the size and contours of the different areas, or in any other features of the environments, and that the divergence in the habits may be directly due to the prevention of interbreeding between separated portions which inevitably differ in average character, especially if they are very small portions.

*Isolated portions differ in varying degrees from the average character of the Species.*

The italicised portion of the passage last quoted attributes to isolation, in stronger language than I should be willing to use, a direct influence in producing divergence in the adjustments on which fertility in the different portions of the species depends. I should prefer to say that in *some species* the adjustments on which fertility depends are so delicate that adjustments producing perfect fertility within one intergenerating portion of the species will not produce fertility in another portion that has been long isolated. I do not make my statements so sweeping as his concerning the divergent influence of isolation on any one class of characters, but I include all classes of inheritable characters, in sexually producing organisms, as coming under its influence. I also insist that the direct influence of isolation in producing divergence is in proportion to the degree of segregation, which varies immensely in different forms of isolation which are equally

complete as preventives of intercrossing. A very stable and homogeneous species may be divided by geological subsidence into two large sections, each represented by a vast number of individuals. In such a case the difference in the average character, and consequently the degree of segregation, of the two sections will be infinitesimally small, and the influence of the isolation thus produced will chiefly consist in its preserving in the different sections any diversities that may arise in the effects of natural selection or of other principles of transformation. The isolation between the land-animals of Ireland and Britain, which Mr. Wallace cites as adverse to my theory, is of this kind. Again, there may be transportation and isolation of very small fragments of a very variable species. In such a case separation may involve a degree of segregation that from the first produces perceptible divergence. Again, the process by which the isolation is produced may be in itself segregative, in that it brings together those endowed in some special way, causing them to breed together and preventing them from breeding with others. This is especially the case with Sexual, Social, and Prepotential Segregation, and in some degree with Industrial Segregation. Isolation thus produced is in its very nature segregative, and would result in divergence if diversity of natural selection did not arise in the different sections of the species. Segregation with divergence may also be produced by natural selection or some other principle of transformation cooperating with some form of isolation that of itself is not perceptibly segregative. As segregation of other than average forms always produces divergence, and without it there is no divergence, I claim that it is the fundamental principle of divergent or polytypic evolution. Natural selection, which is the exclusive propagation of those better adapted to the environment, when it results in the preservation of other than average forms, produces confluent or monotypic evolution; but it is never the cause of divergence, except when cooperating with some principle of isolation in such a way that the two principles produce segregation. Failure to recognize these distinctions prevents Mr. Wallace from understanding my theory, and leads him to represent me as claiming for isolation all that I claim for segregation.

*Incompatibilities arise during Positive Segregation.*

On pages 173-186 Mr. Wallace maintains that "Natural selection is, in some probable cases at all events, able to accumulate variations in infertility between incipient species"

(p. 174); but his reasoning does not seem to me conclusive. Even if we grant that the increase of this character occurs by the steps which he describes, it is not a process of accumulation by natural selection. In order to be a means of cumulative modification of varieties, races, or species, selection, whether artificial or adaptational, must preserve certain forms of an intergenerating stock, to the exclusion of other forms of the same stock. Progressive change in the size of the occupants of a poultry-yard may be secured by raising only bantams the first, only common fowls the second, and only Shanghai fowls the third year: but this is not the form of selection that has produced the different races of fowls. So in nature rats may drive out and supplant mice; but this kind of selection modifies neither rats nor mice. On the other hand, if certain variations of mice prevail over others through their superior success in escaping their pursuers, then modification begins. Now, turning to p. 175, we find that in the illustrative case introduced by Mr. Wallace the commencement of infertility between the incipient species is in relations to each other of two portions of a species that are locally segregated from the rest of the species, and partially segregated from each other by different modes of life. These two local varieties, by the terms of his supposition, being better adapted to the environment than the freely interbreeding forms in other parts of the general area, increase till they supplant these original forms. Then, in some limited portion of the general area, there arise two still more divergent forms, with greater mutual infertility and with increased adaptation to the environment, enabling them to prevail throughout the whole area. The process here described, if it takes place, is not modification by natural selection. The natural selection of which he speaks does not arise till, with each advancing step, a new and complicated adjustment (which introduces the two new forms, each with unabated fertility with its own kind, but with diminished fertility with the other kind) has been attained by some other process. That other process is the one described in the passage I have already quoted from pp. 184-185, where, according to my apprehension, the cause of divergence is more correctly stated than it is in the passage now under consideration. In the latter part of my paper on "Divergent Evolution through Cumulative Segregation" I have shown that the different kinds of incompatibility, preventing complete fertility between incipient species (and there called forms of Negative Segregation), cannot arise except as accompaniments of Positive Segregation in some form; but that, having once arisen in connexion with partial Positive Segregation, they increase

from generation to generation by a law that is quite distinct from natural selection. It was also shown that endowments only partially segregative (as, for example, somewhat divergent habits of feeding), when not concurrent with any forms of cross incompatibility, are liable to be obliterated by crossing; but, when associated with segregate fertility and cross infertility, will increase from generation to generation, even if the mongrels are as well adapted to the environment as the pure forms. I at the same time called attention to the fact that, when associated with some form of partial positive segregation (as divergent habits of feeding or segregative sexual and social instincts), greater vigour of pure forms, as contrasted with the mongrels, would have the same effect as their greater fertility. In other words, Segregate Vigour would preserve a partially segregated variety as effectually as Segregate Fecundity.

*Incompatibilities will disappear unless preserved by  
Positive Segregation.*

Mr. Wallace has given a very instructive computation on pages 181-184; but it does not seem to me to prove, as he supposes, that infertility between the individuals of a species cannot increase "unless correlated with some useful variation," but that it cannot arise, except as a transitory variation, unless associated with some positively segregative principle, causing those to pair together which are fertile with each other. My contention is that, without some positive form of segregation, fecundity and cross sterility can never arise, and that, after it has arisen under segregation, no amount of correlation with useful variation will preserve it if the positive segregation is removed. If, for example, all the species of humming-birds were brought together in one country, and were deprived of all segregative habits and instincts, it certainly would not require many generations to reduce them to one species. If equally adapted to the environment, the species that would succeed in perpetuating itself would be the one represented by the largest number of individuals; or, if several species were entirely cross fertile and were in the aggregate represented by a larger number of individuals than any other similar group of species or than any single species, then the resulting species would be the hybrid descendants of this most numerous group. All the other species would become extinct through failing to mate with "physiological complements."

*Why any need of distinctive Recognition Marks for those whose Ancestors had but one set of Marks?*

An example of one of the effects of divergence being treated as if it were the primary cause of divergence is found on pages 217-228 and 284, where the need of distinctive characters for easy recognition is given as the chief cause of divergence in calls, odours, and colours. The importance of distinctive characters by which the members of a species may distinguish their mates from those of other species cannot be exaggerated; but how does it happen that the descendants of one stock which had originally but one set of such characters have become segregated into groups, needing distinctive marks? By confounding the problem of successive monotypic adaptation with that of coexistent polytypic adaptation the real causes of divergence have been obscured and misapprehended. The diversity of Sexual and Social Selection, which Mr. Wallace in these passages speaks of as natural selection, is due to diversity of sexual and social instincts, which in their turn have been produced by different forms of segregation. For a fuller exposition of this subject I would refer to my paper on "Divergent Evolution through Cumulative Segregation" (Journ. Linn. Soc., Zoology, vol. xx. pp. 234-238). The principles which I have called Sexual and Social Segregation Mr. Wallace has mentioned in several places under the name "selective association" or "selective isolation," but he does not recognize the fact that, whenever this principle segregates forms whose immediate ancestors were not segregated, it must be the direct cause of divergence; and that, when divergent forms that have arisen under Industrial and Local Segregation are brought together through increase of numbers, this principle is often the one cause preserving varieties that would otherwise be obliterated. With plants whose pollen is distributed by the wind, and probably with both vegetable and animal forms whose fertilizing elements are distributed by water, Prepotential Segregation plays the same rôle as the segregative instincts of higher animals. As this principle depends on the greater rapidity with which the male and female elements of the same variety or species combine, as contrasted with the elements of different varieties and species, we might call it isolation through selective impregnation, just as Mr. Wallace has called the instinctive segregation "isolation through selective association." Whatever names we give these two principles, they must be important factors in divergent evolution.

*Segregation produces Domestic Races, why not Species?*

Mr. Wallace seems to be opposed to the idea that some form of isolation is essential to divergence; but in his argument he yields so much that I cannot but think his opposition is largely due to his misinterpreting the theory. Mr. Romanes has mentioned eight or ten forms of isolation, and Mr. Wallace says I have discussed thirty-eight forms; but neither of us claim that these are the only possible forms, nor do we claim that any form of this principle is essential to the transformation of one species into another when the original one disappears in the process. The phrase "new species" as used by Mr. Wallace in the following passage is ambiguous; but the second sentence seems to indicate that he is here discussing divergence as well as simple transformation. He says:—"Most writers consider the isolation of a portion of a species a very important factor in the formation of new species, while others maintain it to be absolutely essential. This latter view has arisen from an exaggerated opinion as to the power of intercrossing to keep down any variety or incipient species and merge it in the parent stock. But it is evident that this can only occur with varieties that are not useful, or which, if useful, occur in very small numbers." . . . (p. 144). Near the end of the same chapter, after presenting arguments in favour of this position, and after reviewing some of the facts which I have presented concerning the divergences of Sandwich-Island land-molluscs, he remarks:—"We have, however, seen reason to believe that geographical or local isolation is by no means essential to the differentiation of species, because the same result is brought about by the incipient species acquiring different habits or frequently a different station, and also by the fact that different varieties of the same species are known to prefer to pair with their like, and thus to bring about a physiological isolation of the most effective kind" (p. 150). Except that he has used "physiological isolation" where I should have used psychological segregation, this last passage is as completely in accord with what I have presented in my paper on "Divergent Evolution" as it could have been if he had copied my statements. But how is this passage and one of similar import on page 185 to be reconciled with his own statement just quoted from page 144? On pages 217, 218, and 226, he bases his argument for the importance of different coloration in closely allied species on the obvious necessity for means "to secure the pairing together of individuals of the same species," if a new species is to be kept "separate from its nearest allies." He here

assumes the fundamental fact on which the theory of segregation rests. All that is wanting is its recognition as a universal principle on which all permanent divergences, whether varietal or specific, necessarily depend. In the formation of domestic variations it is fully recognized; for he says, "It is only by isolation and pure breeding that any specially desired qualities can be increased by selection" (p. 99). If experimental biology shows this to be a constant law, is there any good reason for not applying it in the general theory of organic evolution? Seeing it is admitted that artificial selection, unaided by isolation, is of no avail in producing divergent races, how can it be claimed that natural selection, unaided by isolation, is of any avail in producing varieties and species? Again, as in domestication the segregate breeding of other than average forms always produces divergence, have we any reason to doubt that, when the same process takes place in the grouping of organisms in a natural state, the result will also be divergence?

The discrepancies to which I have referred are, it seems to me, due to deficiencies in the theory which Mr. Wallace maintains in common with many others. These problems that drive the exclusive utilitarian into various inconsistencies, can, I am convinced, be consistently explained by the theory of Divergence through Segregation.

26 Concession, Osaka, Japan.

XV.—*On a Viviparous Caddis-fly*. By J. WOOD-MASON, Superintendent of the Indian Museum, and Professor of Comparative Anatomy in the Medical College of Bengal, Calcutta.

SOME years ago, while studying a series of transverse sections through the body of a Trichopterous insect I had captured at the dinner-table lights, I noticed that the abdomen was crammed from end to end with partially developed ova. On the 25th October last I caught a second specimen of the same species, which also proved to be a gravid female. Remembering my former observation, and having often observed that gravid females of the viviparous forms of Muscidae bring forth their young on falling accidentally into the spirit of the dissecting-dish, I threw the insect alive into a liqueur-glass of whiskey that happened to be ready at hand. The moment that